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# Reviewers Comments on the 5th Symposium and the Status of Fusion Research 2003

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## **Reviewers Comments on the 5th Symposium and the Status of Fusion Research 2003**

**Richard F. Post**

Better to understand the status of fusion research in the year 2003 we will first put the research in its historical context. Fusion power research, now beginning its sixth decade of continuous effort, is unique in the field of scientific research. Unique in its mixture of pure and applied research, unique in its long-term goal and its promise for the future, and unique in the degree that it has been guided and constrained by national and international governmental policy.

Though fusion research's goal has from the start been precisely defined, namely, to obtain a net release of energy from controlled nuclear fusion reactions between light isotopes (in particular those of hydrogen and helium) the difficulty of the problem has spawned in the past a very wide variety of approaches to the problem. Some of these approaches have had massive international support for decades, some have been pursued only at a "shoestring" level by dedicated groups in small research laboratories or universities.

In discussing the historical and present status of fusion research the implications of there being two distinctly different approaches to achieving net fusion power should be pointed out. The first, and oldest, approach is the use of strong magnetic fields to confine the heated fuel, in the form of a plasma and at a density typically four or five orders of magnitude smaller than the density of the atmosphere. In steady state this fusion fuel density is still sufficient to release fusion energy at the rate of many megawatts per cubic meter. The plasma confinement times required for net energy release in this regime are long – typically a second or more, representing an extremely difficult scientific challenge – witness the five decades of research in magnetic fusion, still without having reaching that goal.

The second, more recently initiated approach, is of course the "inertial" approach. As its name implies, the "confinement" problem is solved "inertially," that is by compressing and heating a tiny pellet of frozen fusion fuel in nanoseconds, such that before disassembly the pellet fuses and releases its energy as a micro-explosion. The first, and most thoroughly investigated means to create this compression and heating is to use multiple laser beams, with total energies of megajoules, focused down to impinge uniformly on the pellet target.

To illustrate the extreme difference between the usual magnetic confinement regime at that of inertial fusion, there are twenty orders of magnitude in fusion power density (ten orders of magnitude in plasma density) between the two regimes. In principle fusion power systems could operate at any density between these extremes, if means were to be found to exploit this possibility (we will later mention some such possibilities).

From a governmental funding standpoint, except during the first "exploratory" decades of the research two factors have dominated the way in which funds are allocated for magnetic fusion research. The first of these is the "bandwagon effect," that is, when one or more major research groups begins to invest substantial funds in the construction of a particular type of confinement system other countries and institutions feel compelled

to jump on the bandwagon, at the same time usually downplaying or abandoning other approaches that might, in time, prove superior to the new front-runner. The second factor determining funding levels is how well this front-runner advances the “performance index goal” – usually taken to be the product of ion temperature, plasma density and confinement time –  $n(m^{-3})T(keV)\tau(sec)$ . This criterion, however, tends to resemble a “self-fulfilling prophecy” in that the fusion performance of any system tends to scale up with size (and cost). This is indeed the course, now decades long, that has been followed in the pursuit of the tokamak, where succeeding generations of machines were invariably scaled up in size and cost. As is well known, the tokamak is based on confining the plasma in a doughnut-shaped chamber by magnetic fields that are the superposition of a toroidal magnetic field generated by coils wrapped around the doughnut and the poloidal field arising from megampere-level currents induced in the plasma itself by transformer action or other means. The push for scale-up in size of the tokamak resulted from many observations all of which were consistent with an empirically determined “law” that predicts a steady increase in confinement time with increasing the radius of the confinement chamber. This scaling law is a direct result of the fact that tokamaks operate in a turbulence-dominated regime, that is one where the only consistent way to increase the confinement time en route to net fusion power is to increase the plasma radius, so as to take advantage of the increase in transport time across the confining field. That is, by lengthening the distance (thus the time) required for the turbulence-enhanced diffusion processes to transport the plasma to the chamber wall

Thus the “bandwagon” effect in funding, with the tokamak being the prime example, is further amplified by the sensitivity of the “performance index” to size of the facility. On the international stage only a few countries and/or institutions have been able to retain a more balanced approach in the face of these two pressures. The first of these is Japan, and another of these is the Budker Institute of Nuclear Science at Novosibirsk. In the main, with few exceptions, the other major governmentally funded fusion research centers have given only lip service to approaches other than the tokamak, now the only candidate for a full-fledged attempt to achieve  $nT\tau$  values corresponding to net fusion power. This multi-billion-dollar facility is the internationally sponsored “ITER,” decades-long in planning, and likely a decade or more in construction. Compromises in the design, aimed at reducing the cost, diminish the projected performance, leaving some to question whether ITER will in fact achieve its goal of net fusion release, and even if it does, whether the tokamak will represent an economically viable approach to fusion power.

Turning to the other main branch of fusion research, inertial fusion, a rather different picture emerges. First there has been, to date, essentially only one way to attack the problem of achieving net power gain, that is, through the use of multiple laser beams. Again, the performance index has been increased mainly by simply upping the peak power and the number of the beams. Working against this increase have been problems associated with the design of the pellet targets and with the timing and spatial uniformity of illumination of the targets, required to achieve a uniform compression and heating of the target. Also, the funding picture has been very different. The bandwagon effect seems not to have played a role, and, second, because the technology has defense-related applications the funding has usually been consistent and generous. In the U.S. the NIF (National Ignition Facility) at the Lawrence Livermore National Laboratory, will be the

flagship of the U. S. inertial fusion program. Also, based on decades of experimental preparation with smaller facilities, and extensive research on the target physics, NIF should achieve its goal of plasma ignition with high probability. On the international scene, The LMJ megajoule laser is under construction in France. LMJ has similar capabilities and objectives to those of NIF. As with the tokamak, the path from NIF or LMJ to an economically viable fusion power plant based on lasers is not at all clear.

With the above general comments as a backdrop, we will make some selective comments about the 5<sup>th</sup> Symposium, cast in the light of the international fusion effort. First off, as an examination of the papers presented at the Symposium will show, it is far more eclectic than the larger, internationally sponsored, fusion conferences. There are papers representing virtually the entire gamut of approaches to the fusion problem, from the largest and most costly ones – ITER, NIF, and LMJ – to the smallest explorations of off-the-beaten-track approaches. We will here make no attempt to cover all the work reported, but will select only a few items to illustrate some specific points. We will then conclude with some general comments expressing a point of view with respect to suggested changes and/or additions to the international fusion effort aimed at increasing its probability of success for developing an economically viable source of energy, and also, possibly substantially shortening the time required to achieve that goal, as compared to the present, ITER-dominated, path.

Papers at the 5<sup>th</sup> Symposium (and the international fusion program itself) can be roughly divided into categories that reflect their “philosophical” approach to the problem. For example, virtually all the “closed” field systems, e. g. the tokamak, the stellarator, the reversed-field pinch, and the spheromak are known to suffer from endemic turbulence, as a result of which the cross-field heat transport is orders of magnitude faster than the “classical” (Spitzer) theoretical rate of transport across a uniform magnetic field. The “philosophy” here is to attempt to suppress the most virulent forms of the turbulence and then compensate for the effects of the remaining turbulent modes by scale-up in plasma radius and control of the plasma parameters. This is the path followed by ITER and the new-generation stellarators, the “spheromak” and the reversed-field-pinch. There were papers on all of these approaches at the Symposium.

A second discernable “philosophy” is that exemplified by the so-called “magnetized target” approach. There were several papers at the Symposium on this approach. Here the concept is to carry out a magnetic compression of a “target” plasma at such a high rate and to such a high level of magnetic field that the required time for net fusion power release is shorter than all but the most virulent plasma instabilities. Here the magnetic field, which provides the main compression and heating to ignition, also provides “insulation” for the plasma, but now only needing to function for microseconds to accomplish this end. A distant cousin of the magnetized plasma is the “plasma focus,” based on fast compression by a pinching plasma ejected from a plasma gun,

The third “philosophy” already discussed, is that of inertial fusion described in the papers on NIF and LMJ and other papers at the Symposium, where the goal is to carry out the entire process, compression, heating, and fusion burn on an inertial time scale, i.e. one where no “containment” other than the time to disassemble is required. Even here, however, there are problems of uniformity of irradiation and stimulation of “Rayleigh-Taylor instabilities. These problems arise, whether using the “direct” target method with impingement of the laser beams on the target, or employing the “hohlraum”-based

technique. where the target is illuminated by intense x-rays generated by impingement of the laser beams on the inner surfaces of a "hohlraum." cavity. Some notable departures from the laser-based approaches to inertial fusion were papers on particle-accelerator-based heavy-ion drivers, a potential simpler and cheaper way to achieve fusion power from inertial fusion, and the "Z-pinch" Hohlraum x-ray source, based on the implosion of a cage-like assembly of fine wires, heated and compressed by the "pinch effect to produce a cavity filled with x-rays at very high pulsed-power levels (terawatts). These alternatives to the laser would seem to offer some major advantages in terms of simplicity and cost.

Finally, there were a few papers that were implicitly based on an entirely different "philosophy." That is, attempts to exploit magnetic geometries where theory (and in some cases, experiments) show that the totality of MHD and other instabilities endemic to closed systems are either absent or are suppressed to low levels. These geometries then offer the possibility of fusion-relevant confinement systems with smaller magnetic field intensities and/or smaller physical size and complexity than the closed systems. One such paper concerned the Field-Reversed-Configuration (FRC), stabilized by rotation. Another concerned the Levitated Dipole Experiment, mimicking the earth's dipole field, and predicted to be free of MHD or current-driven instability modes. A third paper concerned special open-ended field geometries that were predicted to be stable against closed-field-type modes. The last of these papers, on the "Kinetic Stabilizer", builds on theory by Ryutov, confirmed conclusively in the Gas Dynamic Trap open-field axisymmetric mirror experiment at Novosibirsk. In the GDT it was shown that the low-density effluent plasma exiting the mirrors, when expanded sufficiently in the outward-flaring field lines outside the mirror, can act as an MHD "anchor," strongly stabilizing MHD interchange modes in the mirror-confined plasma, up to interior beta values of 40 percent. Once thus stabilized the confined plasma showed no evidence of turbulent transport, even in the presence of a substantial high-energy ion component, created by neutral-beam injection. The Kinetic Stabilizer, as it would be applied to an axisymmetric tandem-mirror fusion power system, uses ion beams injected into the end of the "expander." These ions, compressed, stagnated, and reflected, form a plasma in situ in the expander that stabilizes the plug and central-cell plasmas of the tandem mirror, at an energy cost that is calculated to be small compared to the fusion power released in the central cell.

To sum up, the Symposium covered very well the whole spectrum of approaches to fusion now being pursued without regard for whether they were "on the bandwagon" or the minimally supported efforts of groups examining "off-the-beaten-path" approaches. As to comments and general recommendations that might be made, one stands head and shoulders above all the others. That is, internationally and in the U. S. in particular, the funding level for fusion is grossly at variance with the need to solve the fusion problem in the shortest possible time. Considering the economic, environmental, and political-instability problems arising (and soon to arise) from our use of oil and coal, fusion should be given a Level One priority by all the nations involved in the research. Second, although the front-line approach, the tokamak, deserves continued support, it seems clear that it is folly to "put all of ones eggs in one basket," programmatically. In addition to greatly increased funding, a proper balance needs to be restored to the national and international fusion effort, whereby substantial support is given to promising

alternatives, particularly those that are “orthogonal” to the tokamak, in that their physics issues are very different from those of the tokamak, with its endemic sources of turbulence, and its high complexity. In addition to these plasma-physics-related fusion policy issues, there are critical technologies, such as those of high magnetic fields and novel solutions to the “first wall” problem, that need to be tackled in parallel with the broadened “alternates” program.

How long will it take us to “take fusion seriously,” and tackle it as we are tackling, for example, the problem of cancer? That field of research is tackled on a broad front, with generous funding, and with minimal interference from the body politic. Fusion is at least as important, in the long-run, as reducing the onset of cancer.

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